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A NOTE FROM THE EDITOR

Ethel Tobach, *Editor*

Final solutions: Biology, prejudice, and genocide, by Richard M. Lerner. Pennsylvania State Press, University Park, PA, 1992, 238 pp. + xxii.

This issue presents commentaries on a controversial book. As the topic relates to the scientific and societal significance of extant theories about behavioral evolution, it is appropriate for a journal of comparative psychology that includes in its goals the explication of fundamental concepts of human evolution to provide a forum for the ongoing discussion of the relationship between genetics and behavior, between scientific concepts and public policies. As the responses by the various authors indicate, there are many issues that are unresolved and still require examination. Perhaps some member of the community of comparative psychologists who read the journal and have read the book might want to develop material for another issue of the journal on these topics, or some aspect of them.

The inclusion of the article by Sokolov and Baskin offers further material for consideration in the above discussion. The article came about through a discussion I had with Dr. Baskin on one of my trips to the former Soviet Union. In my several trips and discussions with senior researchers, faculty and students engaged in the study of behavioral evolution and genetics, most of whom termed themselves "ethologists," I was not surprised to find that, as in the United States, most of them had no knowledge of Lorenz's personal history. In the course of one such discussion, I was told that there had been an article in one of the popular science journals in the Soviet Union about Lorenz and his experiences as a prisoner of war. Whether or not the story is apocryphal remains for elucidation by historians. According to the story told me, while imprisoned, Lorenz made ink out of various materials available to him, and was writing his book on toilet tissue (a detail that is surely to be questioned in light of the article by Sokolov and Baskin) when a guard discovered this. Because of the guard's respect for Lorenz, he did not report Lorenz's activity to the authorities. A time came when an official visited the camp to inspect its activities. The guard warned Lorenz that he might have to hide what he was doing from the official. However, when the official met Lorenz and became impressed by his scientific background,

the guard told the official what Lorenz had been doing. As a result, Lorenz was transferred to a hotel in Moscow where he was allowed to write his book in comfort with suitable materials. I asked the scientist who told me about this article in the popular journal if it would be possible to obtain a copy of this article. He apparently did not follow through on my request, but when I told Dr. Baskin about the story told to me, he and Dr. Sokolov went about the task in a laudably systematic fashion, with the results published here. There is still a need for a historian who is interested in this issue to make use of the many documents that until now were kept hidden and are now available.

The resolution of other issues raised by the book, the commentaries and the article also remains for historical processes that, as Lamb says, are probably not in the control of the scientific community.

REVIEW

Gary Greenberg
The Wichita State University

Lerner's important book is divided into two parts, each of which deals with a significant issue. The first part presents a critique of genetic determinism espoused by contemporary ethology and sociobiology; the second part is a discussion of "developmental contextualism," Lerner's alternative to genetic determinism. Both parts of this book are also controversial: the first because of its treatment of Konrad Lorenz; the second because of the emotional and passionate attachment many still have to genetic determinism. Some have argued that Lerner's characterization of modern ethology and sociobiology is off the mark and outdated; this is, in fact, the case with several of the reviews in this special issue (Kaye, Lamb, Siegel). However, Lerner's characterization still forms the basis of much thinking in modern ethology and sociobiology, particularly in its new guise of "Evolutionary Psychology" (Caporael & Brewer, 1991; DeKay & Buss, 1992).

The controversy in the first part of the book has to do with the political implications of genetic determinism, which is illustrated by Lorenz's intellectual participation in the Nazi's "final solution." In a paper written in 1940, Lorenz argued for the genetic basis of human behavior and the weakening and subsequent degeneracy that would result as a consequence of interracial breeding; some suggest that that paper presents the argument that "only the state, by controlled breeding, can stop the decline towards degeneracy" (Allen, 1977, p. 82). We are all familiar with the consequences of this point of view. No doubt some would argue that Lorenz's Nazi past is of little consequence to science. Indeed, a rather warm, grandfatherly picture is usually painted of him. I do not agree with this picture; nor does Lerner. It is not only not improper, it is imperative that science have a social conscience.

This aspect of the book was controversial even before it was published, Lerner having been told by publishing officials that it was "too hot to handle" (personal communication). Lerner has been accused of shoddy scholarship because he had not read Lorenz in the original but had,

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rather, depended on numerous secondary sources. The truth is, however, that even though Lerner does read German, because he thought his competency insufficient to deal with nuances, he compared multiple translations of Lorenz and had his colleagues who teach German review the adequacy of those translations by looking at what Lorenz said in German. He did not rely on secondary sources as has been alleged. It should be noted that the translations he relied on have never been seriously challenged. Indeed, when he had the opportunity to do so, Lorenz himself did not challenge the English translation of a passage from his infamous 1940 article (R. M. Lerner, personal communication, April, 1991).

As Lerner points out, ethologists, sociobiologists and evolutionary psychologists see behavior, even human behavior, as programmed by our genes. As we inherit our behavior through our genes, we have bad or aggressive genes, and thus, we are by our very natures a hostile and aggressive species. A somewhat grim and pessimistic outlook about life indeed! In other words, human nature is fixed by our genes and passed on from generation to generation in the same way that eye color is. Although geneticists have abandoned such simplistic ways of thinking about genetic influences, this "genetic essentialism" and its serious social implications still form the basis of much thinking about behavior (Dreyfuss & Nelkin, 1992). But, if not the genes, what then? The answer to this question is the subject of the second part of the book.

T. C. Schneirla was the most important theoretician in comparative psychology at the time of his death in 1968. Among other things he postulated that behavior develops as a matter of the various experiences we undergo throughout the course of our maturation. To change those experiences, then, is to change the resulting behavior. "Are cats rat killers or rat lovers?" asked Zing Yang Kuo, the great Chinese psychologist. The answer is that it depends. Kittens raised with rats out of sight of cats that kill and eat rats never eat rats themselves, *even when hungry*. Having never seen a rat eaten, it is simply not food for these cats. But, that's precisely the point, particularly with respect to human behavior. About this, Ashley Montagu (1962) has said that "The wonderful thing about a baby is its promise" (p. 17).

Lerner has developed this long line of thinking from J. R. Kantor's "interbehaviorism" and T. C. Schneirla's "approach/withdrawal theory" (Lazar, 1974) into a theoretical orientation he calls "developmental contextualism." It is an optimistic conception of human behavior because it puts the burden on experience, i.e., ontogeny and psychology, rather than on genes, i.e., phylogeny and biology. While we can never escape our genes, we can engineer our development, something Skinner never stopped believing. Since behavior develops in a context, changing that context changes the experiential possibilities, thus altering our behavioral repertoires. The significance of this approach to understanding behavior

is that it is heuristic and empirically testable, as Lerner's work has shown over the years; it is, as well, parsimonious. However inviting his approach appears to many, it, too, is controversial. I suspect that this is due to its apparent neglect of biology as an important influence on behavior. But biology is not left out of developmental contextualism; it is fused with psychology.

The controversies generated by this book stem from the passion with which adherents approach genetic determinism and its major heroes. This passion is reflective of the dominant biological perspective in the psychological sciences. If it is not biology, the reasoning goes, somehow it cannot be real. But, as did Kantor and Schneirla before him, Lerner has shown that it is possible (and even desirable) to develop a uniquely psychological way of thinking. This line of thought is based on the philosophy of integrative levels (Feibleman, 1954), which for psychology implies that behavior is an epigenetic phenomenon, an emergent process resulting from the fusion of two levels, biology and psychology. This orientation is preferred to that which substitutes mere biological labels for real explanations. In a sense, I am glad that Lerner's book has stirred the fires of controversy. In dealing with this particular subject matter (nature-nurture) this seems to be the most successful way to get the message across.

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REVIEW

Jerry Hirsch
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My endorsement on the dust jacket of Lerner's book states, "Professor Lerner tells a story we need to know and presents a perspective we need to consider." As Lewontin makes clear in the foreword:

Final Solutions is a book about the extreme horrors that arise when people take seriously the proposition that there are racial and national characters from which we cannot escape. Unlike other books on Nazi race theory, *Final Solutions* extends ideas of determinism beyond the purely biological into the cultural as well. Cultural determinism—the doctrine that our cultural heritage passed down by a process of unconscious acculturation is inescapable—differs only in a trivial mechanical detail from biological determinism, the doctrine that we cannot escape our genes. Both biological and cultural determinism deny essential freedom to human consciousness.

I still believe that these characterizations accurately describe the valuable message conveyed by this important book. So this review now re-endorses my earlier endorsement. That said, I shall comment on both the strengths and weaknesses of the presentation. Its strength lies in the clarity and drama with which past evil and everpresent danger are communicated. There are, however, major weaknesses.

The key person in the story told by Lerner is Konrad Lorenz. As Garland Allen commented in his review of a Lorenz biography,

Nisbett . . . has . . . miss[ed] the main point to be learned from a study of Lorenz's work. . . . Ideas can become deadly weapons when they provide a supposedly objective and rational description for human social behaviors. . . . From his notions that many social behaviors are innate and that humans have an instinct for preserving the "purity of type," all else follows. Lorenz was a respected scientist whose views, inadvertently or not, could be used to legitimize a brutal genocidal

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philosophy. In killing Jews, Aryans were only "doing what comes naturally." . . . Lorenz's case ought to help us see that the prevalence of such theories (not just one theory, but many of similar ilk) can pave the way for a holocaust. It is not a perversion of history to try and learn from the past. (Allen, 1977, p. 84)

Richard Lerner has now written the book for which Garland Allen was calling in 1977 when at the end of his review he dismissed Nisbett's biography of Lorenz as "a great disappointment" for the reasons stated above.

Unfortunately, the reader is led to believe by Lerner as well as by Lewontin and Muller-Hill that Lorenz's contributions to ethology (and ethology itself) have been discredited because he used (or misused) that information in support of Nazi ideology. I agree that his misuse of knowledge, including overgeneralization from animals to man, was tragic. But that is a human tragedy, not a scientific one. Werner Heisenberg's leadership of Hitler's atomic bomb project is, as in the Lorenz case, a personal tragedy, but it does not diminish the importance in physics of the uncertainty principle for which he too received a Nobel prize. Nor is Wagner's music to be rejected either because of his personal life or because the Nazis liked his music.

In reviewing the same Lorenz biography as Allen did, Aubrey Manning makes the important points:

There can be no doubt of Lorenz's key role. . . . He *is* the father of ethology, however many of his children now reject his ideas.

The early papers in which Lorenz developed the concepts of the fixed action pattern, the releaser and the evolution of displays, of action-specific energy and of imprinting, are all the result of dedicated observation and description shot through with brilliant intuitive thinking. The approach to the animal in its natural environment and the concern with function and evolution came as a powerful tonic to behavioural science stultified by white rat experimental psychology.

When we come to look back at Lorenz's contribution in more tranquillity . . . it is *King Solomon's Ring* that will stand out as his great book. Within its easy charm are all the important ideas which make up Lorenz's profound contribution to the study of behaviour. (Manning, 1977, p. 783; also see Marler & Griffin, 1977/1973)

A serious omission from Lerner's discussion is an appreciation of the important correction to Lorenz's too pessimistic ideas about vertebrate (especially primate, including human) aggression that has been provided by Frans de Waal's (1989) superb exposition in *Peacemaking among Primates*. Yes, primates are driven by a strong aggressive motivation to hostile acts, as Lorenz suggested, but there is a countervailing motivation towards reconciliation which, circumstances permitting (i.e., the oppor-

tunity to avoid the fight or flight plight), takes over and preserves the group cohesiveness—an ingredient that was lacking on Monkey Hill in Zuckerman's (1932) classic, but flawed, study of primate aggression and which Lorenz also failed to appreciate.

Chapter 4 on sociobiology appears to have been prepared at the same time as an article written by Lerner and von Eye (1992) on that topic. I was invited to write a commentary that has been published together with their article (Hirsch, 1992). Therefore, I say here again some of what is relevant to both texts.

There is a message from genetics that can clarify our thinking about development—Lerner's main interest—because it delineates what is and is not possible. Developmental scientists think in terms of people who are born, mature and reproduce themselves by contributing children to the next generation, and the cycle repeats every generation. The foregoing statements are focussed on what geneticists call the level of the phenotype. At that level it appears that each generation reproduces itself in the children it contributes to the next generation.

The story is quite different, however, at the level of the genotype. In diploid, bisexual, cross-fertilizing species like man, *no* genotype can replicate itself! In fact, the term "reproduction" becomes misleading if it is interpreted to mean self replication, as appears to be the case in Lerner's version of sociobiology. In a very important sense it is not the individual, but rather the *species* that reproduces. Because of meiosis, none of us can reproduce ourselves genetically, rather we can contribute only a haploid gamete to the reproduction, or continuation, of the species at each conception (Hirsch, 1963). Meiosis is the mechanism that distributes to the genome of the gametes a randomly chosen single homologue (intact or as a cross-over product) from each of our 23 pairs of chromosomes. In this way the chromosome number ($2N = 46$ in man) is reduced to half ($N = 23$) at gametogenesis. This occurs in both sexes at the so-called reduction division of meiosis. Therefore, during the fertilization produced by mating—the union (syngamy) of the two haploid gametes (sperm and ovum), one contributed by each sex—the new individual thus formed, though reconstituting the diploid chromosome set (karyotype) of the species, has a genotype that is markedly different from that of either parent. So, each parent contributes a haploid gamete to the reproduction of the species, but can never reproduce him- or her-self. Therefore, we should not speak of reproduction as self-replication, because that is genetically impossible.

It is good that Lerner articulates his version of the language of sociobiology, thereby enabling readers to appreciate why so many of its claims are incorrect. The sociobiological perspective is characterized as maintaining that

... we have evolved not to produce other people but only to replicate our particular complement of genes. (p. 92)

All genotypes must struggle arduously to include as many copies of themselves in the gene pool as possible. . . .

All genotypes are not equally fit for having their replicates in the pool of genes. . . .

Aggression is the key to getting one's genotype reproduced maximally. (p. 95)

Women . . . remain monogamous . . . to maximize the probability that their relatively few replicates will survive. (p. 102)

. . . genes provide the ultimate basis of our functioning, the replication of our genotype. (p. 103)

. . . the . . . humans in question . . . possess evolutionarily based genetic "directives" for genotype reproduction. (p. 110)

. . . women as genotype reproducers (p. 132)

What is not appreciated by either sociobiologists or the critics is the impossibility of genotype replication.

Lerner's treatment of the question of racial differences in "intelligence" as measured by IQ tests (see especially ca. p. 145, his discussion of Rushton's claims) ignores the fundamental *experimental* analysis and clarification of the nature of test construction by Harrington (1975, 1984, 1988; Hirsch & Tully, 1982), who has cogently demonstrated the impossibility of any test being unbiased. The racial comparisons and their usual interpretation, that are discussed, are without any justification. As Reynolds has recognized, because of Harrington's work "100 years . . . of psychological research in human differences . . . must be dismissed as confounded, contaminated, or otherwise artifactual" (Reynolds, 1980). In other words, as was recognized by the U.S. National Academy of Sciences in 1967 and reported by me in a publication (Hirsch, 1981, p. 8) cited by Lerner: ". . . there is no scientific basis for a statement that there *are* or that there *are not* substantial hereditary differences in intelligence between Negro and white populations" (my emphasis).

A point in need of emphasis is the independence of knowledge from its use or misuse. Knowledge results from the interaction of events with observation, theory (or interpretation) and our perception of distinctions (i.e., distinguishing from previous knowledge information that then becomes new knowledge), even though the use or misuse might not be independent of the knowledge (i.e., some might seek to justify an action [the use or misuse] as being based on the knowledge).

Emotionally I'm strongly sympathetic with Lerner's developmental contextualism concept, that has the innate mechanisms continually in strong interaction with the external context in which development is occurring, but not at the expense of precluding research into the separate influences of the innate and external factors. Johnson, Bolhuis and Horn

(1992) show by experimental analysis that imprinting in chickens involves at least two processes: a process of learning to recognize certain features of various kinds of stimuli and a predisposition (innate) to approach stimuli resembling conspecifics. Starting with a population of *Drosophila melanogaster* manifesting the species typical (innate) negative geotaxis and individual variations in the degree of its expression, we have bred two populations (races) with opposite manifestations of the behavior, i.e., what was formerly species-typical has evolved in response to selection to become population-typical (Hirsch & Erlenmeyer-Kimling, 1961).

My reaction to the discussion in the Epilogue about helping "to ensure that 'it doesn't happen again'" is simple: Only an educated citizenry in a democratic state that excludes any individual or group from a monopoly of power is our safest, but uncertain, protection. The old cliché remains apropos: the price of liberty is eternal vigilance.

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REVIEW

Theodora J. Kalikow
Plymouth State College

Richard Lerner seems to have set himself four tasks in *Final Solutions*: one, to argue that scientific theory, in this case a theory about human nature, has implications for social policy and, in turn, that scientific theory is not immune from social influences; two, to demonstrate the horrors to which one erroneous view of human nature—biological or genetic determinism—led when it informed social policy in the Nazi state; three, to show that this error is alive and well and continuing to do damage; and four, to make the case for a different view of human development—developmental contextualism.

These tasks are worthy. Each of them could have been developed at greater length and each one has been discussed by other writers. For instance, Lerner focuses on Konrad Lorenz as an example of a scientist whose work derived legitimacy from the Nazi state and also supported it. I have written on this too, and Lerner discusses my work at some length. I am not going to comment directly on his discussion, except to say that Lerner has given more evidence to support a thesis I have argued for, and I'm pleased to see it. However, I do want to show the power of the combination of tasks that Lerner has chosen by grappling with the work of Konrad Lorenz once again, this time with a focus on biological or genetic determinism.

Warning about the imminent degeneration of society was a constant theme in Lorenz's work, along with the presupposition that genetic decline was the root cause. But his view of the reasons for decline changed at least three times over a career spanning the middle of our century. In the 1930s and 1940s, Lorenz attributed the cause to domestication-induced mutations in the human genome; in the 1950s and 1960s, the cause was supposed to be simply the removal of natural selection factors from the conditions of civilized life, so that a wider variation in (presumably deleterious) genetic endowments could be reproduced in successive generations. The 1970s and 1980s saw Lorenz attribute the decline of civilization to the fact that the knowledge produced in society far over-

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whelmed the information programmed into the human cognitive/instinctive equipment, so that we could no longer control the forces we had unleashed and might destroy our environment. In each generation biological determinism was presupposed as correct, and failures in the human genome were attributed to different causes.

In every era, too, there was a corresponding social movement to combat the alleged cause. Nazism attempted to eliminate the "cancers" and "dregs" from society. Later, *On Aggression* unleashed a media blitz of "naked apeism," whose underlying thesis was that we had to preserve our instinctive emotional and behavioral equipment in the face of civilization's threats to it. (These threats could be construed to include the Civil Rights and women's movements.) Finally Lorenz became a grand eminence of the German Green party, railing against the evils of an overly complex society in the best Old Testament prophetic tradition.

How explain Lorenz's success, indeed, his celebrity? As Lerner makes clear, he was a scientist whose work, deliberately or not, spoke to the already existing presuppositions of a wider society, who used "scientific" reasons to legitimate and explain the concerns, and who prescribed "scientific" remedies. This can be seen in each of the three eras I have mentioned.

In the first era, Lorenz was young, struggling and not at all famous when he wrote his apologies for National Socialism mixed in with reports of observation, theory and experiment, but what he wrote fit in with the prevailing world view. I have argued elsewhere that Lorenz's use of race-political language might have been a deliberate strategy to gain acceptance for the new science of ethology, which he was consciously building. Besides, there is strong evidence that Lorenz really believed the claims he made. In this connection, Lerner is especially good at showing the continuity of Lorenz's thought with that of other scientists and physicians of the time.

In the 1960s the notoriety of *On Aggression* (and clones by other authors) again signalled that Lorenz's claims echoed what many people were ready to hear. While opponents thought that his ideas were horribly wrong, argued against the determinism implicit in them, pointed out connections with the Nazi genocide, and so on, the commercial success of writers like Desmond Morris, Robert Ardrey and Lorenz himself showed what side the popular world view favored.

In the 1980s, environmentalists in Germany took over Lorenz's concerns with a new twist. Human activity is, after all, responsible for the sad state of the environment. Lorenz gave reasons for this activity that had much more to do with individual human limitations in a complex society than with economics, business or government; truly a prophet that everyone could accept without guilt.

When every generation brings a new reason for decline and a new social

movement to capitalize on it, this is a clue that we have entered the realm of ideology, here defined as the set of presuppositions underlying theories and world views. Teasing these presuppositions out is a useful exercise.

The first presupposition is that decline is a real phenomenon that needs to be explained. Nostalgia is a social disease. For whatever complicated historical, psychological or other reasons, human beings throughout Western history have been prone to thinking that the past was always better, the present generation is no good, the world is coming to an end. Of course, when we have the capacity to destroy the world by nuclear or environmental holocaust, that is real. But it is important to distinguish the desperately real from illusions of cultural despair.

The second presupposition is that a complex set of social-political phenomena—named, incorrectly, as decline—can be explained and corrected on the basis of the individual organism. But most of the changes (excluding disasters) that affect human behavior on a mass scale are changes on the level of ideas or of social organization. Blaming the genetic decay of individuals for the “ills” of the social and political world just does not work.

The third presupposition is biological or genetic determinism itself. This is one of the controlling metaphors of the 20th century and of 20th century biological sciences. While its roots are very old, it was not until the 19th century that evolution theory was available to begin the strengthening of biological determinism into a controlling metaphor for human nature. But, while Darwinism claimed that human nature had been shaped by biological processes over millennia, there was no clear understanding of how traits were passed on to succeeding generations. Even Darwin left room for the influences of environment. But the inception of genetics at the beginning of the 20th century soon led to a view that human nature depended totally on the genes.

Lerner describes the results eloquently:

From the perspective of genetic determinism, it is not what any of us does in life, not the environment we encounter, that is involved in the manifestation of our particular set of behaviors. Instead, those behaviors are functional outcomes of our biological heritage, our genes. Thus, what we do, what we become, is built into us at conception, is biologically predetermined. As a consequence, those who are most able to compete, who have achieved and mastered the world, and who therefore occupy positions of power and prestige, have those places in society because of what has been inherited, not because of what has been encountered or learned. (p. 13)

As Lerner demonstrates, presuppositions, metaphors and myths matter. They affect the doing of science; scientific results influence social policy.

The sequence can iterate itself. Not only have we had a Holocaust, but biological determinism continues to do great damage to groups like African-Americans and women.

Other damage can result to science and society. For example, as R. C. Lewontin argues in the "The Dream of the Human Genome" (1992, pp. 31–40), biological/genetic determinism is a guiding presupposition of the Human Genome Project, currently planned to take many millions of dollars to complete, with goals that may be elusive or even unreachable. What are the social implications, the consequences for individuals, and the effects on the scientific enterprise, if much money and time are spent on a project whose conceptual underpinnings are seriously flawed and whose promises therefore cannot be kept?

As I have argued elsewhere (1990), what we need is a new post-modern myth or metaphor for what it is to be human in the world. We need to construct a new ideology. Part of it will be what Lerner argues for, developmental contextualism. R. C. Lewontin, in his introduction to *Final Solutions*, calls this the central point of Lerner's work. Here is a further description of developmental contextualism from Lewontin:

Even the organism does not compute itself from its DNA. A living organism at any moment in its life is the unique consequence of a developmental history that results from the interaction of and determination by internal and external forces. The external forces, what we usually think of as "environment," are themselves partly a consequence of the activities of the organism itself as it produces and consumes the conditions of its own existence. Organisms do not find the world in which they develop. They make it. Reciprocally, the internal forces are not autonomous, but act in response to the external. Part of the internal chemical machinery of a cell is only manufactured when external conditions demand it. . . .

Nor is "internal" identical with "genetic." . . . The variation between sides [of a fruit fly] is a consequence of random cellular movements and chance molecular events within cells during development, so-called "developmental noise." It is this same developmental noise that accounts for the fact that identical twins have different fingerprints and that the fingerprints on our left and right hands are different. A desktop computer that was as sensitive to room temperature and as noisy in its internal circuitry as a developing organism could hardly be said to compute at all. ("The Dream of the Human Genome," p. 34)

Developmental contextualism has been around for awhile. Beginning in the 1950s, the eminent comparative psychologist T. C. Schneirla and his school argued for it against Lorenz's approach. Developmental psychologists like Lerner may have adopted it because earlier theories do not do justice to all the phenomena they have observed. In any era,

ingredients for more than one ideology can be available, rather like traits of an organism that become useful when conditions change.

Of course, developmental contextualism is more than a presupposition for a world view. It is a theory like biological determinism, and it may even represent a next stage in the latter's development. That would depend on transcending the simple dichotomy of nature vs. nurture, while still using the many important results of modern biological science. But this means giving up a view of human nature that we in the 20th century have responded to so many times in so many guises. Lerner has brought together powerful arguments why we should do so. Perhaps his book will be an impetus for a new synthesis.

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REVIEW

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In his new book, Richard M. Lerner is determined that the lessons learned from biology's shameful involvement in the horrors of Nazi genocidal programs must not be lost and that the hateful doctrine of biological determinism never again be permitted to justify oppression, inhumanity, and ultimately mass murder. Such vigilance is essential now, because he believes that the ideology of biological determinism has indeed been resurrected, this time in the guise of sociobiology, and that it too may well be used for "politically pernicious, fascist purposes" (p. 195).

To combat this evil, Lerner launches a two-pronged attack. First, by attempting to demonstrate the essential continuity, even identity—in both scientific concepts and policy implications—between the kind of biological determinism which led to the gas chambers and the sociobiology of today, Lerner seeks to discredit this biological perspective on moral and political grounds. To believe that there is something fixed in our nature, be it blood or genes, which makes us who we are and can become is, Lerner insists, a "pessimistic" (p. 9) and brutal ideology that has long been used to justify the exploitation, persecution, and even slaughter of others and the maintenance of the social status quo. Second, Lerner seeks to challenge biological determinism and, in particular, its contemporary incarnation as the "selfish gene" on scientific grounds. Sociobiologists, Lerner tells us, essentially ignore the role of the environment in human development and insist that "what we do, what we become, is built into us at conception, is biologically predetermined" (p. 13). But genes, Lerner reminds us, "do not exist in a vacuum" (p. 156), nor do they "represent fixed and immutable blueprints for behavior and social standing" (p. 180). They are instead "plastic entities that are fused with a complex context" (p. 180) constituted by the human organism's physiological, social, and cultural environments, thereby generating "an infinity of behavioral outcomes" from "the same hereditary contribution" (p. 157).

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Lerner terms this more scientifically accurate perspective "developmental contextualism," a perspective which he has derived from the work of T. C. Schneirla and Ethel Tobach. But not only is developmental contextualism more scientifically correct than biological determinism, and environmental determinism too, it is more politically and morally correct as well. If biological determinism has "necessarily xenophobic and ruthlessly selfish implications for society" (p. 103), the implications of developmental contextualism are unfailingly humane, liberating, and optimistic, celebrating human diversity and enhancing the human condition (pp. 169, 186): "the developmental contextual perspective demonstrates how human existence can be improved and in fact 'recreated' across the entire span of human life . . ." (p. 3). Combining "good science" with "good service" (p. 169), developmental contextualists will be able to plan programs and initiate interventions to transform "an entire cohort of people" in "positive" ways (pp. 82, 169, 189-190).

Written with considerable moral fervor, *Final Solutions* does succeed in demonstrating the scientific inadequacy of the 'genes as blueprint' metaphor and its personification as a ruthless Chicago mobster. Following the work of Robert Proctor and Robert Jay Lifton, Lerner reminds us, all too painfully, that good scientists may inadvertently or even intentionally contribute to bad causes. Nevertheless, I consider Lerner's book to be a seriously inadequate and even self-defeating critique of "biological determinism." As a critic of biological determinism and sociobiology, I have considerable sympathy for many of Lerner's concerns, but I fear that the distorted and caricatured picture of "the enemy" which he presents is so glaring that his work will be too easily dismissed.

As a work of scholarship, this is a book that was written fifteen years too late. Recycling the work of others (primarily Proctor, Lifton, Choroever, Lewontin, Gould, Kalikow, and Müller-Hill), Lerner does not realize that the straw men that he seeks to bludgeon—Social Darwinism, ethology, and sociobiology as proto- or crypto-fascist ideologies of biological determinism—have been dead for years. What representative sociobiologist would recognize himself or herself in Lerner's portrait as someone who denies the interaction of genes and environment in human development and conduct (p. 103); who believes that women, blacks, and the poor are biologically inferior (pp. 125, 128-129); and who believes that the existing social order is biologically ordained, unchangeable, and essentially just (pp. 14, 19)?

Regrettably, Lerner's analyses of individual theorists are no more reliable. Konrad Lorenz, despite his Nazi past, is not the advocate of killing and ruthless oppression that Lerner portrays him to be (compare Lerner's patched-up quotation from pages 251 and 48 in *On Aggression* with the actual text). Nor does he believe that good and evil are genetically determined and advocate extermination of the ethically inferior (pp. 85, 87), which is how Lerner interprets Lorenz's 1974 statement that "I

strongly hold that ethical inferiority of individuals due to heredity or to bad upbringing . . . is indeed a reality which has to be taken seriously." The truth about Konrad Lorenz is bad enough without such distortions.

Readers of Richard Dawkins, Melvin Konner, and E. O. Wilson will be similarly surprised to find them portrayed as celebrants of "blind, ruthless, militant aggression" (pp. 93-95). Those, however, who are unfamiliar with sociobiology would never know from Lerner's account that it is altruism not aggression which has been the central theoretical interest of sociobiology. And in a particularly egregious example of misreading, Wilson's lament about the possible loss of our humanity in the sense of a loss of altruism, cooperation, and creativity (1975, p. 575) becomes in Lerner's hands a warning against "the destruction of humanity" unless ruthless eugenics programs are enacted against the poor and the ignorant. No amount of moral fervor, no matter how understandable, can justify such misrepresentations.

It is indeed ironic that a scholar such as Lerner who criticizes the "faulty reasoning" and "inadequate scientific methods" of biological determinists for ignoring the importance of social and cultural contexts, for trivializing complex phenomena (p. 109), for "using findings from a few studies to make generalizations" about a whole class of phenomena, and for "making uncritical comparisons among studies" of very different things (p. 148) should be guilty of the same in his historical analyses and contemporary critiques. To lump together Plato, Spencer, Haeckel, Freud, Lorenz, Wilson, and Herrnstein, Social Darwinists, Eugenicists, Nazi racial hygienists, ethologists, and sociobiologists, with no appreciation of either the context or specific content of their work or the profound differences in their perspectives, is bad *social* science and constitutes "serious violations of the rules of scientific debate" (p. 148).

In his presentation of "developmental contextualism" as a more sophisticated scientific perspective, Lerner is certainly on surer ground. Yet his belief in its necessarily positive and humane implications, however comforting, requires careful consideration. Are well-intentioned programs "to develop positive and/or valued social behaviors" (p. 82) so benign; is the desire of experts to "enhance the human condition" through the manipulation of the masses such an unambiguous good? Does the "actualization" of some undefined "human potential" (pp. 170-186) automatically create a good human being and a good society? These questions are too important to be ignored. That they are ignored further confirms that *Final Solutions* is far more a work of polemics than one of scholarship and serious reflection.

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NAZIISM, BIOLOGICAL DETERMINISM, SOCIOBIOLOGY, AND EVOLUTIONARY THEORY: ARE THEY NECESSARILY SYNONYMOUS?

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Richard Lerner's new book, *Final Solutions: Biology, Prejudice, and Genocide*, is a powerful and troubling treatise. It weaves together several topical strands into a direct, clear, and compelling argument. The chief strength of the book lies in its focus on a single aspect of Nazi ideology (biological determinism), the role played in the maintenance of that ideology by medical and biological scientists, and Lerner's warnings about those he views as the contemporary successors of these scientists. Unlike Lerner's other contributions to the scholarly literature, this book is less a psychological treatise than it is a polemical history of some behavioral sciences in the twentieth century. Lerner's argument is provocative, clearly reasoned, and demands consideration by social scientists, humanists, and those who would avoid both the repetition of the past and our ignorance of its costs and lessons. The timeliness of the book is underscored by the current spectacle of genocidal mayhem in Bosnia, complete with the specter of officially endorsed rape in the service of ethnic hatred and racial pollution. To facilitate scholarly debate concerning Lerner's book, my emphasis in this review is on some troubling aspects of Lerner's argument, rather than on the many strengths of this important book.

Lerner's volume begins with the observation that the Nazi ideology advanced by Hitler had its philosophical roots in a cadre of biological scientists dating back into the previous century. These scientists gave voice to the notions of racial superiority and the importance of racial purity that lay at the heart of both Nazi ideology and the chilling actions (the Holocaust) enacted in its name. Like other recent authors, such as Robert J. Lifton, Lerner points out that Hitler was intellectually and pragmatically supported in the implementation of this genocidal policy by a variety of medical and biological scientists, who confirmed the im-

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portance of racial purity and the absolute moral correctness of eliminating "impure elements," such as Jews, gypsies, and other "Non-Aryan" races.

Lerner's primary focus is on Konrad Lorenz, an Austrian biologist whose contributions to the understanding of animal behavior earned him the Nobel Prize in 1973, even though, as Rajecki, Lamb, and Obmascher (1978) wrote shortly thereafter in an appraisal of his most widely cited scientific work: "[Lorenz's] provocative notions stimulated an enormous amount of research, the result of which is that all of Lorenz's postulates on imprinting can be viewed as incorrect" (p. 418). Lerner reviews several articles written by Lorenz during the Nazi era in support of his argument that Lorenz prostituted his science in the service of Nazi ideology and in particular played a prominent role in promulgating the notion that individual behavior, attainment, and potential are simply manifestations of biological destiny.

It is this notion of "biological determinism"—that we are what our genes make us—that comes in for the harshest criticism by Lerner. It is his contention that biological determinism did not die with the Nazis. Indeed, the fact that the first three behavioral scientists to be awarded the Nobel Prize for Medicine (Lorenz, Tinbergen, Von Frisch) all studied the biological bases of behavior implicitly underscores for him the continuing belief on the part of many social scientists and philosophers that biological determinism is a viable and defensible position.

In fact, much of Lerner's book is focused on "the sociobiologists," whom Lerner sees as the contemporary successors of the German biological determinists who pandered to Hitler in the 1930s and 1940s. Lerner is at pains to argue that contemporary sociobiology contains the seeds of the sorts of prejudice that fueled Nazi philosophy. As a result, it could provide scientific justification for present and future evils that are qualitatively similar to those undertaken in the name of Naziism and racial purity earlier in the century.

Lerner's book is at its weakest in its designation of sociobiology as the epitome of contemporary scientific evil, however. Lerner takes several texts written by well-known sociobiologists to exemplify the dangers implicit in sociobiological theory (which he represents as a monolithic theoretical edifice). In his view, the cited writings of such authors as E. O. Wilson, Melvin Konner, and Philippe Rushton (respectively a biologist, an anthropologist, and a psychologist) exemplify the problems inherent in sociobiology. Such an inference from a few examples to an entire class of scientists is troubling to me. To argue that sociobiology is inherently evil because several "sociobiologists" have adopted positions that are troubling or incorrect seems a little like arguing that all painters should be eschewed because Hitler was a painter. Is nuclear physics benign and morally pure because Einstein was a pacifist, or wicked and immoral because Teller was a manipulative zealot? Neither, surely. In

fact, there are many scientists who might be called sociobiologists whose positions are nowhere near as deterministic as Wilson, Konner, and Rush-ton. Ironically, when Lerner presents developmental contextualism as his alternative to sociobiology, he cites work on sex reversal in coral reef fish—a phenomenon that exemplifies some of the most important socio-biological research of the last two decades (Charnov, 1982)! Unfortunately, Lerner fails to take advantage of the opportunity to demonstrate the diverse strains in the scholarly literature as it pertains to these troubling and difficult issues. Perhaps this was too much to ask of a book that was already overburdened. Sociobiology is in fact a broad approach to the study of behavior, not a single simple ideology. Sociobiologists attempt to understand how biological factors (most notably, inclusive fitness) influence behavior. They are especially concerned about evolutionary processes that have taken place over long periods of time and might have produced tendencies evident at the level of large groups or populations. Most sociobiologists have sought to explain how individual fitness depends on the constraints imposed and potentiation implied by others' behavior and by aspects of the physical environment. Few study or speculate about human behavior and many, if not most, well-respected sociobiologists emphasize the important role of environmental factors in shaping the potential manifestations of biological tendencies. Indeed, in George C. Williams' (1966) classic book on sociobiology (written long before E. O. Wilson coined the label "sociobiology") substantial emphasis is placed on the need to view biological tendencies in the context of environmental potentiation, support, and constraint. In my view, the most exciting and lasting recent contributions to our understanding of the evolution and genesis of behavior have been made by psychologists, ethologists, and behavioral ecologists adopting exactly this approach. In addition, many contemporary exponents of biological determinism are not sociobiologists. Few responsible behavioral biologists or social scientists adopt biological determinism as their model and it is misleading to argue that "sociobiology" is synonymous with "biological determinism."

Lerner provides many frightening examples of the ways in which scientists misuse their science and their status as scientists in pursuit of nonscientific ends. Doubtless, these abuses will continue, as will the efforts by nonscientists to misappropriate and misuse the words, analyses, and positions of scientists whose goals are nobler than or different from their own. Indeed, the ability to distinguish among bad science, the misuse of science, and the abuse of the scientific label by scientists pursuing nonscientific ends will remain an important and difficult issue for contemporary society. Much of Lerner's book is in fact concerned with scientists who have perverted the scientific method and misapplied the label "science" to efforts that are more political than scientific. This issue is one to which science has yet to develop a response. As Harlow,

Gluck, and Suomi (1972) wrote 20 years ago, "One cannot generalize but one must. If the competent do not wish to generalize, the incompetent will fill the field." Today, magazines, newsletters, newspapers, televised talk shows, and a growing phalanx of neoscientific journals are filled with the pronouncements of self-proclaimed experts. There are no easy ways for society, the media, or science, for that matter, to distinguish among these competing notions and individuals, sanctifying some as more "scientifically" defensible than others. In fact, one could argue that hucksterism is the sole determinant of popular impact and that science, as understood by scientists, is increasingly irrelevant to social discourse.

Lerner is at his best demonstrating the chilling implications of biological determinism and the awful role played by those scientists who provided the scholarly underpinning for unspeakable actions in the pursuit of biological purity. It is for this that his book demands serious attention, not only by scientists, but by many others as well. The lesson for the future is that science, philosophy, and ideology are not independent of one another. Together, each can be appropriated in the service of either good or evil. Biological determinism is just one notion that has been and surely will be misused. *Caveat civilis*.

One last, depressing thought deserves brief articulation as well. Social scientists tend to believe in causal relationships between ideologies or attitudes and behavior. There is no reason to believe that venal human behavior is less likely to occur in the absence of evil ideology, however. Over their recorded history, humans have manifested a seemingly infinite number of ways of wreaking havoc on one another, and the presence or absence of ideology does not appear to have altered these tendencies unduly. Even though racial hatred and other manifestations of intense prejudice are despicable, therefore, we cannot expect the elimination of such rationales for evil behavior to improve our common miserable fate.

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REVIEW

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Final Solutions: Biology, Prejudice, and Genocide by Richard M. Lerner is an important book about the way in which biological determinist views of human behaviour have shaped social policy and have been used to justify persecution of certain groups, even to the ultimate horror of extermination of Jewish people by the Nazis. Lerner traces genetic explanations for human behaviour from Darwin, through scientists of the Nazi era to contemporary sociobiologists. He finds a continuous thread of ideas and alerts us to the political dangers of the impact of such thinking on social attitudes and policies.

Lerner discusses in detail the era of history in which scientists voiced arguments for the genetic inferiority of Jewish people. This ideology was co-opted to justify, on the one hand, elevating the status of Aryans and, on the other hand, ridding society of "bad genes" by sending Jews to the gas chambers. Lerner presents evidence that these ideas did not die with the end of World War II, but rather they have continued to form the basis of contemporary sociobiology. Indeed, today we see sociobiological arguments incorporated into the political platforms of the extreme right, such as the National Front in UK and the neo-Nazis in various European countries. As Lewontin points out in a foreword to the book, right now "Europe is in the process of turning back the pages of history": national chauvinism is rising again, "foreign" minorities are being blamed for the level of unemployment and attacked, racism is being violently expressed, and more. Sociobiological explanations for human behaviour justify these actions. Belief that there are "good genes" and "bad genes" and that certain groups of people are genetically inferior to others has, throughout history, justified the most barbaric social practices and political programs. It is essential that we become familiar with the source of these ideas and their influence on society, as they so strongly shape it today.

Lerner begins by discussing not only biological, or genetic, determination but also social, or cultural, determinism. The former is "nature

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over nurture" and the latter "nurture over nature." He sees either of these views as too extreme, and both as scientifically inadequate. The book elaborates on these scientific inadequacies, but primarily those of biological determinism. The inadequacies of cultural determinism are merely touched upon by some poorly discussed examples of oppressive social policies that have stemmed from belief in environmentalism, such as the "Cultural Revolution" in the People's Republic of China. Lerner's own position is that we should avoid the extremes of the nature-nurture debate and see the interaction, or rather *fusion*, of the biological processes which occur within the person with the social context which occurs outside the person. His theory of *developmental contextualism* is discussed in the last chapter of the book.

But, first Lerner traces the relationship between biological determinist thinking and racism, pointing out that for at least two thousand years of recorded history social actions have been carried out on the basis that certain people are inherently different (have something in their blood or, later, in their genes) and considered to be less than human. Consequently, it was argued that they deserved to have inferior social and economic status, to suffer persecution or even death. In the mid-nineteenth century science entered into this line of thinking. The most important scientist at first was Charles Darwin with his theory of evolution by natural selection. Social Darwinists incorporated his theory of survival of the fittest into social policy. In fact, some cultures were seen to be "fitter" to survive over others, and the use of certain groups as slaves was justified by seeing them as less fit, and less than human.

There was a progression of these ideas through to the scientists of Nazi Germany and on to present-day sociobiology. Lerner states that biological determinists are not necessarily racist or politically conservative, or adherents of Nazi ideology, whereas Nazis *are* racist and *in addition* they adopt the ideology of biological determinism in order to legitimize their political views. He accepts what sociobiologists say of themselves when, as many do today, e.g., Richard Dawkins, they claim that they are not politically conservative, let alone fascist. That is a fair and open-minded position, and it clearly has merit if we define racism in terms of practice. Yet, one might ponder whether it is possible to be a "theoretical racist" or whether a scientist could be called racist if his or her ideas can be used to support racist acts and policies. However, Lerner remains magnanimous by saying that the scientists themselves are not directly at fault, but rather genetic determinism is co-opted to serve the political agenda of fascism, and that if such biological ideology did not already exist fascists would have invented it.

The book gives a comprehensive analysis of how biology was applied to politics prior to and during the Nazi era, beginning with the influence of Ernst Haeckel in the late nineteenth and early twentieth century. Haeckel built on standard Darwinian ideas to put forward a "scientific"

(in quotes, because the reasoning was flawed) basis for the ideal of the German *Volk*. He categorised people into different “races” and saw these as separate species. Haeckel’s views were taken up by the German anti-Semitism movement and implemented by the German Racial Hygiene Movement. It is frightening to read how scientists and physicians of the time became part of the “new science” of racial hygiene in the lead up to Hitler’s Germany. They were cornerstones in the path from prejudice to genocide. Hitler successfully engaged the cooperation of vast numbers of German physicians, who joined both the Nazi party and the SS. The aim was to revitalise the German “race” by purification of the genes, and that meant exterminating the Jews and some other social groups considered to be genetically inferior.

Lerner devotes an entire chapter to the ideas of Konrad Lorenz, who was actively writing during the Nazi era and went on to lay the basis of sociobiology, and also to receive the Nobel Prize. Lerner explains how the works of Lorenz are the chief link between present day sociobiology and biological determinism of the 1930s. If you hold an image of Konrad Lorenz as the white-haired, kindly gentleman strolling a country lane followed by a group of goslings that are imprinted on him, you are in for a shock. Through a series of citations of Lorenz’s writings, Lerner shows that Lorenz used the terminology of the Nazis during their era and that his thinking was congruent with Nazi ideology. In 1938 Lorenz wrote of the high value of species-specific and innate social behaviour patterns and how these are the backbone of “racial health and power.” He proceeded to say that health of the whole *Volk* depended on the elimination of “invirent types” (cited on p. 61 of the book), which like the cells of a tumor threaten the whole body of the *Volk*. This, and other cited examples, paint Lorenz as clearly following Nazi ideology of the time. Apparently, these views were never relinquished throughout his life, although they were toned down. By clever quotation of appropriate passages from Lorenz’s writing, Lerner illustrates the continuity of Lorenz’s beliefs, beliefs which developed into the “new” subject of sociobiology in the 1960s.

From his earliest writings, Lorenz considered that domestication introduced degenerate genes into a species because it removed the forces of natural selection. Throughout his life he adhered to the notion that, similar to other animals, human social problems were a result of a reduction of natural selection acting to eliminate “bad” genes. In 1974 he stated that domestication had eroded humans’ sense of the normal, their ability to discriminate pathological from nonpathological, and that this was leading to the moral deterioration. Lerner believes that this position did indeed lead Lorenz to support the “final solution,” even though he attempted to mask this view in the post-World War II area. In other words, to Lerner there was an evident Nazi-era/post-Nazi-era continuity in the writings of Lorenz. For this analysis alone the book is worth

reading. After all, Lorenz is considered by many to be the father of Animal Behaviour, and he was awarded the Nobel Prize!

Contemporary sociobiologists accept the core ideas of Lorenz and they have built on them to formulate their own theories. Chapter 4 of the book is an excellent summary of sociobiology from its basic assumptions to its ramifications into aggression, sex roles, IQ and so on. The works of Wilson, Dawkins, Konner, van den Berghe and Barash receive special attention. To these sociobiologists, the genes (or "replicator units") are the driving force of behaviour, and indeed all of society: according to Dawkins, humans are only "survival machines" programmed by the genes, which (or should we say, who?) are selfishly programmed to get themselves into the next generation. All human interaction, all culture and political systems are merely "by-products of the genes." Here we find another form of justification for aggression (genes must compete and so increase their inclusive fitness) and for natural selection which leads to the elimination of certain genotypes.

Sociobiology extends its rubric to include explanations for sex differences in behaviour and morality. Women and men are seen to differ in their potential for transmitting their genes in the future. Because men make less biological investment in the next generation (they do not bear the offspring and their gametes are smaller in size), they are said, by sociobiologists, to increase their fitness by mating with as many women as possible. Women, on the other hand, should optimize survival of the children which they bear and limit their mating to carefully chosen men. Thus, present-day sex roles are justified: men out in the world seeking their fortunes, women tied to the kitchen sink and to raising children. Beyond that sociobiology justifies greater aggression in men (more gametes and more competition) and explains polygamy, hypergamy and double standards of sexual morality for men and women. Similarly, Nazi philosophy encouraged Aryan women to reproduce, and relegated unmarried women and Jews to the same subordinate category.

Lerner does not leave discussion of contemporary sociobiology here, but goes on to discuss the misinterpretations of the concept of "heritability," particularly as used in the IQ debate, pointing out that heritability is a statistical estimate of genetic variability, not a commonality, as many wrongly deduce. Heritability scores refer to differences between people, not the extent to which a characteristic is either genetically or environmentally determined *within* a person. Other words used by sociobiologists, such as "optimization" and "homology," are also frequently misinterpreted when they are applied to social policy, and the book discusses these problems.

A congruence is drawn between the biological ideas of the scientists of the Nazi era and contemporary sociobiologists. Parallel views are held by both about the roles of women and Blacks, culminating today in the claims of J. Phillippe Rushton that Blacks are a subspecies, less evolu-

tionary advanced than Whites. His nonsensical claims are based on reproduction strategies, modelled on false assumptions and take no cognizance of sociological data. Thus, Lerner takes us through a history of ideas, sketching their roots and links with stark, menacing reality. The so-called scientific theories are stripped of their cloaks of respectability and stand naked with their flaws exposed. With Lerner we share the concern that the message of these sociobiological theories might actually be aimed at a political, not a scientific, audience. And yet, we further contemplate, they are extolled by members of the world's leading scientific institutions (Harvard and Oxford, for example). Of this we must be aware, for fear that history might repeat itself.

It makes depressing reading, but Lerner does not leave us here. Rather, he moves on to present his view of developmental contextualism, in which the genes are not considered to be the primary cause of behaviour. Instead, they act in the context of the environment. Genes and environment are seen as having equal influences on behaviour. Some genes are expressed in only certain environments: nature and nurture are considered to be mutually permissive and mutually constraining in their influence on behaviour. He suggests that genes and environment do not simply *interact*, but that they *fuse*. That is, genes and environment are not considered to be two independent entities which interact, but rather they are intermeshed components involved in *dynamic* interactions.

The same genes express different behavioural patterns in different environments, and the environment is in dynamic register with the individual, changing throughout the life-span and actively acted upon by the individual. Individuals actively shape their environments and so contribute to their own context. Contrast this to the Social Darwinist position of the genes determining specified social roles, and it is easy to see how differently these positions affect the making of social policy. The book is indeed valuable reading, particularly in the present climate in which genetic determinism is, yet again, being co-opted for social/political purposes. My only criticism is that the book is North American-centric; some excellent books and papers, e.g., by Steven Rose and S. A. Barnett, have been written on this topic by people outside of North America, but these have not been cited.

THE HOLOCAUST AND BIOLOGICAL DETERMINISM: BEYOND "JUST BECAUSE"

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Carolyn L. Crowley
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In the introduction to this important, well-written and passionate book, Richard Lerner recounts his grandmother's stories about the Holocaust. He tells of his own frustration as a child with her inability to explain how the Holocaust could have occurred—how so many of his family (and others) could have been terminated in the crematoria of the concentration camps—and her ultimate answer: "... just because." The question haunted him: "... my attempts to find a better answer have been a central part of my professional and scholarly career" (p. xvii). The book represents the author's lifelong struggle to understand and construct a theory of how it happened and his quest to go beyond his grandmother's answer of "just because."

It is an impressive piece of work, with a few—very human—flaws. He tries to unearth the roots of Nazi ideology and finds them in various writings stemming from the paradigm of biological determinism. In doing so, it's as if he's knitting a sweater. He starts with a pattern or ground plan (a linear connection between Konrad Lorenz and the Nazis, Sociobiology, Sexism and contemporary racism). While knitting, he revels in the intricate and rich and detailed designs in the sweater. As soon as he has worked a design, he knits more background color as quickly as he can until he gets to the next intricate design, on which he can lavish detail and style. Much of the passion is in the designs. The passion is also expressed in the desire for no loose ends. He snips, trims, tucks, and folds. Neatness counts ... in a sweater! But history isn't neat, linear, integrated, or compact. History has lots of loose ends. Lerner's skill in condensing and connecting material and ideas is impressive and lets him make clean, neat, and persuasive arguments. However, in order to produce such neat connections, he oversimplifies some—and simply ignores other—material. For example, his characterization of current research and theory focussing on biological inputs in development is misleading

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in that some of the most recent, powerful and persuasive arguments go unmentioned. His characterization of contemporary behavioral genetics theory (subsumed under the paradigm of biological determinism) is too simplistic. Lerner, in effect, sets up a straw man. He argues against Plomin's and Scarr's positions in their articles published in the early 1970s in which they posit a "reaction range" of potential development set for individuals by their genetic inheritance; these fit more cleanly into Lerner's argument. With good reason, psychologists like Lerner are not persuaded by attempts to make strong statements based on simple genotype-environment correlations. However, in their more recent work, Plomin and Scarr are not making simple genotype-environment correlational statements. Rather, they argue that individuals select and construct their own environments based upon heritable characteristics.

In Chapters 2 and 3 Lerner manages to transport the reader not only to a different place and time, but to a different mindset. He allows us a glimpse into cultural myths and the growth of a movement (Nazism and the extension of eugenics in prewar Germany). He paints an exquisite picture of Lorenz, the flesh and blood person, full of European charm, and his times. Lerner here shows mastery of his craft, demonstrating impressive Steven-Gould-like mustering of evidence from creatively diverse sources.

In Chapter 3 the author also savages Lorenz, the ethologist and writer. While many of Lerner's attacks may be justified, the fevered pitch in which they are delivered frequently counteracts their effectiveness. At several points he constructs arguments from flimsy and insubstantial evidence, and his reasoning is self-contradictory. For example, Lerner first characterizes Lorenz's writings as following the clear lines of Nazi ideology: "... One may question, however, whether the similarities [between Lorenz's ideas and Nazi party ideology] Kalikow finds are as vague as Richards portrays" (p. 69). In contrast, a few lines later he characterizes Nazi ideology as incoherent and lacking any continuity: "... but there is little reason to expect that the hodgepodge of concepts, the opportunistic twisting of the motley set of ideas, that constitutes the corpus of Nazi ideology should show a neat and logical pattern of influence" (p. 69). His suggestions that (1) the link between Lorenz and Nazi ideology is clear and coherent, and (2) Nazi ideology is hodgepodge are inconsistent and the result of an annoying selective levelling and sharpening.

Lerner's plausible argument connecting Lorenz's writings to Nazi ideology is rendered less convincing by its tone of religious fervor, an anti-Nazi crusade, a holy war against biological determinism and anyone tainted by association with the paradigm. The bad guys all wear black hats. For example, in March of 1974, Vic Cox in an article in *Human Behavior* entitled "A Prize for the Goose Father" either misquoted from a 1940 article by Lorenz or there was a typographical error: "ethnic"

was substituted for "ethical." In a (published, 1974) letter to the Editor to correct the error, Lorenz wrote:

... I beg you to realize that changing ethical into ethnical makes me appear a rabid racist, which I never was. I never believed in any ethnical superiority or inferiority of any group of human beings, though I strongly hold that ethical inferiority of individuals due to heredity or *bad upbringing* [italics ours] ... is indeed a reality, which has to be taken seriously. (p. 87)

In the very next paragraph Lerner claims that this letter clearly indicates that Lorenz held to the idea that a group of humans are inferior by virtue of their ethical heredity, and thus should be eliminated!

Although Lorenz thus insisted that he was not a racist, claiming that he had never believed there was a group of humans who by virtue of their *ethnical* heredity are inferior, he *did* believe in 1974, and in the Nazi period, that there was a group of humans who by virtue of their *ethical* heredity are inferior. It is this group—the moral imbeciles and dregs discussed in 1940—that should be eliminated, Lorenz believed. Such a fine conceptual distinction about who is and who is not to be the target of such 'special treatment' (to use the Nazi euphemism for extermination) provided little comfort to the men, women, and children who were sent to the gas chambers and crematoria. (pp. 87–88)

Things are black and white. Lorenz wears a black hat—no ifs, ands, or buts. Because he maintained the importance of genetic contributions to behavior, Lerner would have us conclude that *all* else is suspect.

In Chapter 4, Lerner discusses heritability and clarifies the limitations of the term as well as its misuse by psychologists. As Lerner aptly points out, heritability is not a measure of inheritance; it is a population statistic reflecting an estimate of genetic variability, not of commonality. The discussion is excellent. He borrows from Gould's analogy of the "Just so stories" to describe the tautology of natural selection. He pinpoints the flaws adroitly in Dawkins' (1976) notion of "the selfish gene." In essence, the selfish gene frees us from moral constraints and justifies inequity. It is a subtle but dangerous extension of the biological determinist argument.

Lerner is at his best when he paints the historical pictures. Emotion is useful and perhaps essential in providing texture; paintings have impact because they contain "hot knowledge," they embody Vygotskian spontaneous concepts. When he develops and tries to sustain arguments—"cold knowledge" embodied in scientific concepts—emotion gets in the way and he falters.

In Chapter 5, Lerner's portrayals of the situations of women and blacks in Nazi Germany are superb—rich, pithy, depressing, full-blooded. In

discussing the importance of childbearing in the Aryan women's domestic role, Lerner unearths the Honor Cross of German Motherhood:

The Nazis' emphasis on women as genotype reproducers was so great that they gave awards—the Honor Cross of German Motherhood—to Aryan women who fulfilled their role as producers of new and National Socialist citizens—a bronze medal for having four children, a silver medal for six children, and a gold medal for eight children. (p. 132)

In contrast, in discussing the social treatment of blacks argued for by some biological deterministic theories, he focusses his scathing attacks on a most unworthy opponent. The ideas put forth by J. P. Rushton are so ludicrous that they hardly deserve notice, let alone lengthy treatment.

In addition to his talent for re-constructing the texture of an historical setting, Lerner is able to extract crucial aspects of that setting and reflect on them. He raises the uncomfortable question of moral responsibility in science, i.e., that science is not value-free, but rather, is an inherently moral (or immoral) activity, in a particularly poignant way.

Many of us lost family during the Holocaust. That it was an almost unimaginably horrible time and set of deeds is irrefutable. Nazi ideology, now, in 1993, is thought of and referred to as a compact, ruthless, cold, and consensually agreed upon body of uniform ideology and consistently heinous actions. But back then, in the 1920s, while the wisps of thought and doctrine were in the air, there was no such consensus about, or finely honed definition of, "Nazi ideology."

Chapters 6 and 7 are devoted to a presentation of developmental contextualism, Lerner's alternative to biological determinism, and its implications for social policy. While he argues persuasively for its power, this particular form of developmentally based systems theory isn't novel. Urie Bronfenbrenner's ecological theory (with its nested micro-, meso-, and macro-systems), metatheory (Pepper), systems theory (von Bertalanffy, Prigogine), and family systems theories (Minuchin) have all been around for a while. Lerner packages them nicely, and his contrast of developmental contextualism to deterministic paradigms is excellent, but it isn't new.

The proposed circumplex model, which has appeared in many of Lerner's recent papers, includes life, the universe, and everything. The reader should be warned a priori that it reflects a dispersive metaphor and stresses scope and comprehensiveness, rather than precision. Like the Health Belief Model that drives so much of the research in behavioral medicine (you need to at least mention it to get funded), the circumplex model is so inclusive that you can't possibly disagree with it; so vague, that virtually any research study can be conducted as long as you mention that "... we know that we are looking at one only small piece of a larger model ..." in the discussion section.

Summary

"Just because" is profoundly unsatisfying for most of us, both as human beings and as scientists. However, once you have exhausted scientific method, theories and explanations, unless you choose to believe in a malevolent higher power, about the only remaining alternative is chance. In some profound way, the Holocaust was not rational. Then—and now—it makes no sense; it's neither just nor fair. It reflects, in part, the randomness in our lives. We don't want to believe—as Einstein could not—that God plays dice with the universe. We try desperately to extract meaning (that may not be there) or impose meaning on random events . . . and sometimes we can't.

Lerner is a scientist, looking for clean, connected, rational lines in a tangled web of history that is only partially ordered (perhaps even "chaotic"). He's "forcing." We would feel better if it all fit neatly into a coherent picture. But complex human events are rarely, if ever, that simple. Perhaps the author should have let some of the loose ends remain as such.

While the book is not flawless, students of human behavior and the human condition need to read this important, scholarly work. In a curious way, Richard Lerner did not "choose" to write this book, nor did he "want" to write this book—Richard Lerner "needed" to write this book. He should be applauded for doing so.

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DÉJÀ VU, DIALECTICS, AND THE CONSTANCIES OF CONTROVERSIES INVOLVING THE NATURE-NURTURE ISSUE: REFLECTIONS ON THE REVIEWS OF *FINAL SOLUTIONS: BIOLOGY, PREJUDICE, AND GENOCIDE*¹

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My first book, *Concepts and Theories of Human Development*, was published in 1976. A few months later the first review of the book appeared. It was negative and, to me, devastating.

A very senior colleague (who, as I recall, was about the age I am at this writing and, as a consequence, does not seem quite so senior now) tried to console me. He argued that the content of the review was less important than the fact that the book was reviewed per se. He suggested that, within a short time, people would forget the content of the review, but that they would recall that they had seen my work mentioned. In addition, he added, other reviews would appear and, he promised, there would be diversity of opinion.

He was right. Several other reviews were published, and I was relieved when the majority of them were favorable. I knew some people (for example, my mother) would focus only on the positive essays. Still, I feared some people would only read or remember the negative reviews (for example, members of a tenure and promotion committee).

I wished that in some way, one that would not be construed as either self-serving or self-promotional, someone would put all the reviews together in one place. Readers of these pieces would recognize that the diversity of opinion that (probably inevitably) existed across the reviews meant that my mother's opinion, that my scholarship was of unquestionable value, was not tenable. However, readers would also see that books such as *Concepts and Theories of Human Development*, ones that dealt with the core issues of development and most centrally with the nature-nurture issue, elicited from equally competent and distinguished

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scholars a range of often contradictory assertions and evaluations. I wished, therefore, that readers would see that the contradictions formed a dialectic. Perhaps they would then conclude that a key point in ascertaining the value of a book that engaged the nature-nurture issue was whether it provided a synthesis that moved the controversy to a higher plane of clarity, precision, or scientific or societal utility.

Now, some 17 years and more than 25 books later, much of my wish has been fulfilled. I am honored that so many renowned scholars have taken the time to comment on, clarify, and offer corrections to my presentation in *Final Solutions*. I am grateful to all of them, even those with the most negative opinions of the book. Indeed, it is only because I am fortunate enough to have both the negative and the positive appraisals of my work appear in the same journal issue that the value of the book—as a vehicle moving the controversies surrounding the nature-nurture issue to a more superordinate, integrated level of discussion—can be evaluated. Of course, it is for readers of the book *and* of the preceding reviews to decide if *Final Solutions* has this value. However, in the hope of facilitating this evaluation it may be useful for me to point to, if not synthesize, the contradictions in appraisals that are found across the reviews.

However, this is where my wish is not realized. By my own estimation, I am afraid that it is not readily evident that a higher level of synthesis has in fact been attained. To those familiar with the history of the debates involved in the nature-nurture issue (see, for example, Aronson et al., 1970; Gottlieb, 1983, 1992; Gould, 1980; Kuo, 1976; Lehrman, 1953; Lerner, 1976, 1986; Schneirla, 1956, 1957; Tobach, 1981; Tobach et al., 1974; Tobach & Greenberg, 1984) it will seem that the range of comments applied to *Final Solutions*—that is, the character of the controversy surrounding the nature-nurture issue in 1993—is not markedly discrepant from the quality of the debates that occurred in 1976, when *Concepts and Theories of Human Development* appeared. Indeed, because of this constancy it is apt to note the oft-cited phrase of Yogi Berra that “it seems like déjà vu all over again.”

The preceding reviews of *Final Solutions* raise at least five issues that were addressed in 1976 in *Concepts and Theories of Human Development* and in the second edition of the book, published in 1986. Several of these issues are closely interrelated. However, for purposes of exposition it is useful to treat them separately.

Issue 1: Genetic Activity and the Question “How?”

The first, and perhaps superordinate, issue pertains to how genes function, how genes contribute to behavior and development (Anastasi, 1958; Schneirla, 1956). Both editions of *Concepts and Theories of Human Development* (hereafter labeled *Concepts*) present a developmental con-

textual view of genetic functioning. This perspective is based on Schneirla's (1957) levels of integration notion and on his and Tobach's concepts of organism-context interaction (Tobach, 1981; Tobach & Schneirla, 1968), or fusion (Tobach & Greenberg, 1984), and constitutes a "developmental systems" (Ford & Lerner, 1992) view of genetic activity. Both editions of *Concepts* and *Final Solutions* adopt this developmental systems perspective.

This view is the antithesis of genetic determinism; this latter view is—as explained in both *Concepts* and *Final Solutions*—a conception that rests on the beliefs that: (a) genes and context are separable levels of organization; and (b) genes, as compared to context, exert an independent and primary influence on behavior and development. In opposition to genetic determinism, a developmental systems perspective emphasizes that genes and context are part of an inseparable, or fused, system. This perspective has recently been described by Gottlieb (1992):

The ultimate aim of dissolving the nature-nurture dichotomy will be achieved only through the establishment of a fully developmental theory of the phenotype from gene to organism (p. vii). . . . Individual development is characterized by an increase of complexity of organization—i.e., the emergence of new structural and functional properties and competencies—at all levels of analysis (molecular, subcellular, cellular, organismic) as a consequence of horizontal and vertical coactions among its parts, including organism-environment coactions. . . . Horizontal coactions are those that occur at the same level (gene-gene, cell-cell, tissue-tissue, organism-organism), whereas vertical coactions occur at different levels (gene-cytoplasm, cell-tissue, behavioral activity-nervous system) and are reciprocal, meaning that they can influence each other in either direction, from lower to higher, or from higher to lower, levels of the developing system. . . . The cause of development—what makes development happen—is the relationship of the two components, not the components themselves. Genes in themselves cannot cause development any more than stimulation in itself can cause development. When we speak of coaction as being at the heart of developmental analysis or causality what we mean is that we need to specify some relationship between at least two components of the developmental system. (pp. 161–163)

The facts of genetic activity support this development systems perspective: The actual role of genes (DNA) is not to produce an arm or a leg or fingers, but to produce protein (through the coactions inherent in the formula $\text{DNA} \leftrightarrow \text{RNA} \leftrightarrow \text{protein}$). The protein produced by the DNA–RNA–cytoplasm coaction then differentiates according to coactions with other cells in its surround. Thus, differentiation occurs according to coactions above the level of DNA–RNA coaction (i.e., at the supragenetic level). (Gottlieb, 1992, pp. 164–165)

The developmental systems perspective can find no middle ground—no compromise or synthesis—with conceptions that rest on the separation of genes from context. Both sociobiology and behavioral genetics are, at their core, such conceptions.

It is my representation of this core, and my belief in the impossibility of compromise with it, that evoke the criticisms found in the reviews of *Final Solutions* by Kaye, by Lamb, and by Siegel and Crowley. Each of these scholars expresses strong reservations about my views of genetic activity and, as well, about my descriptions of genetic determinism—both in general and specifically in regard to sociobiology and behavioral genetics.

Kaye seems annoyed by what he regards as my intemperate and moralistic writing, a product he believes of “political correctness.” He sees my characterization of the genetic determinist position as “distorted and caricatured” (p. 147). Similarly, Lamb and Siegel and Crowley believe that my ideas about either sociobiology (Lamb) or behavior genetics (Siegel and Crowley) are too categorical, too superficial, and insufficiently nuanced.

Lamb argues that I fail to appreciate that sociobiology is “a broad approach to the study of behavior, not a single simple ideology,” but then goes on to define this entire area of study in general terms, that is, as involving an

... attempt to understand how biological factors (most notably, inclusive fitness) influence behavior. They are especially concerned about evolutionary processes that have taken place over long periods of time and might have produced tendencies evident at the level of large groups of populations. (p. 151)

I cannot disagree with Lamb’s generalization about the focus of sociobiology. His words are virtually identical with those I use in *Final Solutions* to make the same point, but he does not make reference to the fact that evolutionary biologists and population geneticists, such as Gould (1980) and Lewontin (1981; Gould and Lewontin, 1979) have criticized sociobiologists’ treatment of evolutionary processes as imaginary reconstructions of evolutionary history—what Gould (1980) has labeled “Just So Stories.” He also does not note that the other evidentiary bases for sociobiology—the identification of homologies and the reliance upon heritability analyses—have been seen as equally problematic (Hirsch, 1970, 1990; Lerner & von Eye, 1992; Wilson, 1980).

In this regard, reviewer Hirsch notes that “It is good that Lerner articulated his version of the language of sociobiology, thereby enabling readers to appreciate why so many of its claims are incorrect” (p. 138). However, it is precisely in regard to one of these claims—involving the use of the results of heritability analyses as evidence for sociobiological interpretations of genetic activity—that Siegel and Crowley find fault with *Final Solutions*.

Siegel and Crowley indicate that my treatment of the topic of heritability analysis and thus my "characterization of contemporary behavioral genetics theory (subsumed under the paradigm of biological determinism) is too simplistic" (p. 159). They imply that I fail to understand that scholars such as Robert Plomin and Sandra Scarr

... are not making simple genotype-environment correlational statements. Rather, they argue that individuals select and construct their own environments based upon heritable characteristics. (p. 159)

Siegel and Crowley's characterization of the work of Plomin and Scarr is precisely correct, but, as a consequence, the reviewers are "hoisted on their own petard": it is exactly because behavioral geneticists use what they themselves (Plomin, DeFries, & McClearn, 1980) depict as a *descriptive*, sample-dependent, non-generalizable, population statistic for *explaining* individual behavior and intraindividual change that the reliance of behavioral geneticists on heritability estimates is an egregiously flawed procedure (Hirsch, 1970, 1990; Lewontin, Rose, & Kamin, 1984).

Thus, the comments about the issue of genetic activity forwarded by Kaye, Lamb, and Siegel and Crowley are simply wrong. The reviewers are mistaken if they believe there is—at least from a developmental contextual, or developmental systems, perspective—any scientific merit in theory or research based on views of genetic activity (e.g., sociobiology, behavioral genetics) that separate genes from context (Gottlieb, 1970, 1983, 1992; Lehrman, 1953; Schneirla, 1956; Tobach, 1981). Simply, genes "do not work," genetic activity does not occur, in the manner that would be required to validate the conceptual bases of sociobiology or behavioral genetics. In other words, these fields are grounded on a counterfactual view of genetic functioning.

Although many social and behavioral scientists have yet to understand or accept the basic facts of genetic activity, or grasp the implications of these facts for invalidating extant sociobiological or behavioral genetic approaches, among geneticists the developmental systems character of genetic activity is "common knowledge." Thus, reviewer Greenberg, commenting on the flawed reasoning and scholarship involved in genetic determinist conceptions, notes that:

Although geneticists have abandoned such simplistic ways of thinking about genetic influences, this "genetic essentialism" and its serious social implications still form the basis of much thinking about behavior. (p. 134)

In support of Greenberg's view about the rejection of genetic determinist conceptions by geneticists, it is useful to note that molecular geneticist Mae-Wan Ho (1984, p. 285) has indicated that:

Forever exorcised from our collective consciousness is any remaining

illusion of development as a genetic programme involving the readout of the DNA "master" tape by the cellular "slave" machinery. On the contrary, it is the cellular machinery which imposes control over the genes. . . . The classical view of the ultraconservative genome—the unmoved mover of development—is completely turned around. Not only is there no master tape to be read out automatically, but the "tape" itself can get variously chopped, rearranged, transposed, and amplified in different cells at different times.

Similarly, molecular and cell biologist R. C. Strohman (in press), in a review of *Final Solutions* and of Gottlieb's (1992) above-noted book, prepared for another journal, notes that:

Many experimental biologists outside of the biomedical-industrial complex are just now coming (back) to grips with the facts of epigenesis; with the profound mystery that developmental biology is; with the poverty of gene programs as an explanatory device; and with a crisis defined by the realization that an increasingly deficient theory of developmental genetics is the *only* theory currently available. The question remains: if biologists are starting to learn this lesson, will the psychologists be far behind?

I must agree with Strohman (in press) that it is still not clear when psychologists will fully incorporate a developmental contextual/developmental systems perspective about genetic activity into their theory and research. Certainly, however, some psychologists have done this (e.g., Gottlieb, 1992; Tobach, 1981; Tobach & Greenberg, 1984), and as a consequence of their work I must also agree with Gottlieb's (1992) conclusion about the developmental systems nature of human development:

In fact, there is no other way to envisage the manner in which development must occur if a harmoniously functioning, fully integrated organism is to be its product (pp. 165–166). . . . [G]enes are part of the developmental system in the same sense as other components (cell, tissue, organism), so genes must be susceptible to influence from other levels during the process of individual development (p. 167) . . . [and] another important feature of developmental systems is that causality is often not "linear" or straightforward. (p. 169)

Issue 2: Are Genetic Determinist Ideas Viable in Contemporary Science and Society?"

Across the two editions of *Concepts* I stressed that genetic determinist views of human behavior and development were influential in contemporary social and behavioral science. I discussed several instances of such views, ones associated with what Gottlieb (1970, 1983) terms a pre-determined epigenetic perspective. However, I placed emphasis on theory

and research in the study of racial differences in intelligence (or, more precisely, in IQ test scores), and focused on the work of Jensen (1969, 1973) and Burt (1966), as well as on (what were then) more recent contributors to the genetic/family resemblance literature (e.g., Bouchard & McGue, 1981). As a consequence of this first emphasis, I also stressed the heritability literature. Finally, in my attempt to explicate the work of T. C. Schneirla to what I presumed would be a readership largely from the field of human development, a third emphasis was on the work of Konrad Lorenz (e.g., 1940, 1963). Sociobiology was treated only briefly in the two editions.

One reason that I decided to write *Final Solutions*, however, was to expand on my discussion of sociobiology and to draw what I saw as the link between this field and those on which I had focused in *Concepts*. In my view, this link was predicated on a common adherence to genetic determinism. In addition, as had the other instances of genetic determinist thinking that I had discussed, I believed that sociobiology was not only influencing the work of scientists but, as well, was influencing both science policy and social policy. A key purpose of *Final Solutions* was, then, to discuss this *current* instance of the association between genetic determinist ideas and social policy.

It appears that on precisely this point the reviewers divide. However, the very fact that there is diversity of opinion among such a distinguished set of scholars, representing several distinct disciplines—about the contemporary relevance of biological determinist thinking in general, and about sociobiology in particular—supports the argument in *Final Solutions*: genetic determinist thinking/sociobiology is still controversial within contemporary scholarship. Indeed, and as evidenced in some of the preceding reviews, there is validity for Greenberg's observation about "the emotional and passionate attachment many still have to genetic determinism" (p. 133).

Thus, and to illustrate the diversity of perspective among the reviewers, while Kaye contends that "Lerner does not realize that the straw men which he seeks to bludgeon—Social Darwinism, ethology, and sociobiology as proto- or crypto-fascist ideologies of biological determinism—have been dead for years" (p. 147), Greenberg describes my work as "A critique of genetic determinism espoused by contemporary ethology and sociobiology" (p. 133), and goes on to say that "Lerner's characterization still forms the basis of much thinking in modern ethology and sociobiology, particularly in its new guise of 'Evolutionary Psychology'" (p. 133).

Other divisions exist among the reviewers in regard to the importance of genetic determinist/sociobiological thinking for contemporary science and society. For instance, Siegel and Crowley do not believe it was necessary for me to devote any attention to the sociobiological ideas of Rushton (e.g., 1988), a Canadian psychologist who—as I describe in *Final*

Solutions—claims that Africans represent an evolutionary atavism. Siegel and Crowley indicate that “The ideas put forth by J. P. Rushton are so ludicrous that they hardly deserve notice, let alone lengthy treatment” (p. 161).

I agree that Rushton’s ideas are ludicrous. However, I also believe they are dangerous. Indeed, it is precisely the point of my historical analysis that we cannot let such ideas go unchallenged; we cannot let silence be misinterpreted as tacit agreement. Ludicrous and even lunatic ideas have found their way into social policy before—recall the history associated with Hitler’s (1925) *Mein Kampf*. I agree with the point emphasized by Hirsch at the end of his review: “The price of liberty is eternal vigilance” (p. 140).

Moreover, several other comments by the reviewers provide support for the importance of my historical analysis for contemporary science and society. For instance, Lamb indicates that:

Lerner’s argument is provocative, clearly reasoned, and demands consideration by social scientists, humanists, and those who would avoid both the repetition of the past and our ignorance of its costs and lessons. (p. 149)

Lamb goes on to note that:

The timeliness of the book is underscored by the current spectacle of genocidal mayhem in Bosnia, complete with the specter of officially endorsed rape in the service of ethnic hatred and racial pollution. (p. 149)

Rogers concurs with Lamb’s assessment, observing that “The book is indeed valuable reading, particularly in the present climate in which genetic determinism is, yet again, being co-opted for social/political purposes” (p. 157).

Issue 3: Is My Account of the History of Genetic Determinist Thinking Appropriate?

If diversity of opinion existed across the reviews on the contemporary relevance for science and for society of sociobiological thinking, then no less of a division occurred with respect to the legitimacy of my analysis of the presence across history of ideas of genetic or, more broadly, biological determinism. For instance, Kaye, once again critical of my work, employs some of my own writing in *Final Solutions* to indicate that:

To lump together Plato, Spencer, Haeckel, Freud, Lorenz, Wilson, and Herrnstein, Social Darwinists, Eugenicists, Nazi racial hygienists, ethologists, and sociobiologists, with no appreciation of either the context or specific content of their work or the profound differences in

their perspectives, is bad *social* science and constitutes “serious violations of the rules of scientific debate.” (p. 148)

In turn, however, Kalikow sees both validity for, and merit to, my analysis of historical continuities in genetic determinist ideology. She notes that “In each generation biological determinism was presupposed as correct, and failures in the human genome were attributed to different causes” (p. 142).

She goes on to stress that:

When every generation brings a new reason for decline and a new social movement to capitalize on it, this is a clue that we have entered the realm of ideology, here defined as the set of presuppositions underlying theories and world views. Teasing these presuppositions out is a useful exercise. (pp. 142–143)

Among all the instances across history of the links between genetic determinist ideology in science and in society, one is of particular concern to Kalikow: the science and politics of Konrad Lorenz. The career of Lorenz is also a focus in *Final Solutions*. A discussion of the reviewers’ comments regarding my presentation about Lorenz is pertinent to the general issue of the appropriateness of my historical analysis. However, because both *Final Solutions* and its reviews focus separately on Lorenz, it is useful to treat his work as an independent issue.

Issue 4. Is My Discussion of the Career of Konrad Lorenz Appropriate?

There are at least two dimensions to the comments of the reviewers regarding my discussion of Konrad Lorenz: Science and Politics. The reviewers divide in regard to whether I have accurately represented one or both of these dimensions.

Hirsch, for example, sees Lorenz’s Nazism as a personal shortcoming, but one that did not affect the quality of his science. He states that what I depict in my account of Lorenz “is a human tragedy, not a scientific one” (p. 137). Nevertheless, Hirsch goes on to note that I was *not critical enough* of Lorenz’s science, that is, that I failed to note an important scientific shortcoming in Lorenz’s ideas:

A serious omission from Lerner’s discussion is an appreciation of the important correction to Lorenz’s too pessimistic ideas about vertebrate (especially primate, including human) aggression that has been provided by Frans de Waal’s (1989) superb exposition in *Peacemaking among Primates*. (p. 137)

Lamb also criticizes Lorenz’s science. Citing an earlier appraisal of the validity of Lorenz’s science, he notes that:

Konrad Lorenz, an Austrian biologist whose contributions to the understanding of animal behavior earned him a Nobel Prize in 1973, even though, as Rajecki, Lamb, and Obmascher (1978) wrote shortly thereafter in an appraisal of his most widely cited scientific work: "[Lorenz's] provocative notions stimulated an enormous amount of research, the result of which is that all of Lorenz's postulates on imprinting can be viewed as incorrect." (p. 418)

From my discussion in *Final Solutions* and, earlier, in the two editions of *Concepts*, as well as in my above remarks regarding "Issue 1," it is clear that I believe that Lorenz's science—insofar as it rests on genetic determinist conceptions—is irreparably flawed. In addition, I believe I have also been clear that I think Lorenz was personally flawed as well.

Kaye disagrees. He notes that:

Konrad Lorenz, despite his Nazi past, is not the advocate of killing and ruthless oppression that Lerner portrays him to be (compare Lerner's patched-up quotation from pages 251 and 48 in *On Aggression* with the actual text). (p. 147)

Kalikow, however, has a different opinion of what may be gleaned about Lorenz's character from *On Aggression*. She indicates that:

On Aggression unleashed a media blitz of 'naked apeism,' whose underlying thesis was that we had to preserve our instinctive emotional and behavioral equipment in the face of civilization's threats to it. (These threats could be construed to include the Civil Rights and women's movements.) (p. 142)

She goes on to describe that:

In the 1960's the notoriety of *On Aggression* (and clones by other authors) again signalled that Lorenz's claims echoed what many people were ready to hear. While opponents thought that his ideas were horribly wrong, argued against the determinism implicit in them, pointed out connections with the Nazi genocide, and so on, the commercial success of writers like Desmond Morris, Robert Ardrey and Lorenz himself showed what side the popular world view favored. (p. 142)

Despite this division among the reviewers about Lorenz's character, I believe that there are two facts that make the argument far from moot. First, as discussed in *Final Solutions* and earlier in several papers by Kalikow (e.g., 1978, 1983), Lorenz was a Nazi. Second, he published several papers between 1938 and 1943 calling for precisely the sorts of actions to which Kaye says he was not committed—despite the Nazism that Kaye acknowledges. These papers are also discussed in Kalikow (1978, 1983) and in *Final Solutions*.

I must be very blunt here. I do not apologize to my colleagues if I cannot forgive a person—despite whatever level of scholarly eminence

he might have achieved—who willingly associated himself with a regime committed to and enacting genocide. Accordingly, I find fault with Siegel and Crowley's comment that:

Things are black and white. Lorenz wears a black hat—no ifs, ands, or buts. Because he maintained the importance of genetic contributions to behavior, Lerner would have us conclude that *all* else is suspect. (p. 160)

These reviewers fail to understand that it was not Lorenz's genetic determinist thinking that was the basis for the "human tragedy" that Hirsch depicts. Rather, it was his Nazism. I do believe that Lorenz's science was "wrong", but, and as I argued in *Final Solutions*, independent of his science was his politics. And Lorenz's politics were evil. In my world view, there are no "good Nazis."

My stance here, and my phrasing, may strike some readers as inappropriate for a scientific journal. However, I must agree with Greenberg that "It is not only not improper, it is imperative that science have a social conscience" (p. 133).

My position here leads to another fault I find with the views of Siegel and Crowley. These reviewers indicate that my

... suggestions that (1) the link between Lorenz and Nazi ideology is clear and coherent, and (2) Nazi ideology is hodgepodge, are inconsistent and the result of an annoying selective levelling and sharpening. (p. 159)

Nazi ideology was a motley set of ideas that, together, were even more poorly reasoned and ludicrous than those of Rushton (1988), which Siegel and Crowley thought I would have done better to ignore. However, the nature of Nazi ideology made it all the more easy to find some way to link one's own ideas with this ideology, especially when—as was the case with Nazism and Lorenz's ideas—a common overlap with then contemporary views about biology existed.

Accordingly, and for a host of possible reasons, Lorenz commingled his ideas, ones that certainly had roots in his work prior to the beginning of the Nazi regime in 1933, with this Nazi ideology, and he published these "integrated" ideas in "journals" that were political tools of the Nazi State. Nothing about this series of events strikes me as inconsistent or, in fact, surprising, if one stresses—as I choose to do—that the person involved in this history was a willing, card-carrying member of the Nazi Party.

The review by Rogers supports my stance. She agrees with my contentions that:

Lorenz used the terminology of the Nazis during their era and that his thinking was congruent with Nazi ideology ... [that Lorenz was] clearly following Nazi ideology of the time ... [and that] there was an

evident Nazi-era/post-Nazi-era continuity in the writings of Lorenz. (p. 155)

Issue 5. Is Developmental Contextualism a Scientifically Useful Theory of Human Development?

Much of the diversity in the reviewers' opinions about *Final Solutions* rests, I believe, on their evaluations of the scientific use of developmental contextualism. If so, I am not surprised. The range of opinions expressed in reviews of both editions of *Concepts* reflected different perspectives about the theoretical and empirical use of this perspective. Indeed, the range experienced in respect to *Concepts* is evident in the present set of reviews.

For example, Greenberg indicates that:

The significance of this approach to understanding behavior is that it is heuristic and empirically testable, as Lerner's work has shown over the years; it is, as well, parsimonious. (pp. 134–135)

Although I am grateful for this characterization of my work, it is necessary to point out that quite an alternate appraisal is also found in the reviews. While labeling developmental contextualism as "the circumplex model," Siegel and Crowley claim that developmental contextualism is a completely dispersive model (but see Lerner & Kaufmann, 1985), one that is too inclusive and, by implication, empirically unfalsifiable. They state that:

The reader should be warned a priori that it reflects a dispersive metaphor and stresses scope and comprehensiveness, rather than precision. Like the Health Belief Model that drives so much of the research in behavioral medicine (you need to at least mention it to get funded), the circumplex model is so inclusive that you can't possibly disagree with it; so vague, that virtually any research study can be conducted as long as you mention that "... we know that we are looking at one only small piece of a larger model ..." in the discussion section. (p. 161)

It is hard to reconcile this portrayal of developmental contextualism with either Greenberg's appraisal or with the several lines of work, from laboratories around the world, that are productively using this theoretical view. That is, ideas associated with developmental contextualism are being used to derive and test numerous models, ones making precise (and falsifiable) predictions about how changing *relations* between specific aspects of person and context should and should not relate to specific features of behavior and development. Some exemplary cases in point

are found in Baltes (1987), Brooks-Gunn (1987), Clausen (1993), Eccles (1991), Elder (1974, 1979), Magnusson (1988), Perlmutter (1988), and Stattin and Magnusson (1990).

CONCLUSIONS

I began *Final Solutions* by describing a sad and frightening incident, one involving a story told to me by my Grandmother. It may be appropriate to end my discussion of the reviews of the book with a story about a more pleasant interaction I had with her.

During the summers my Grandmother and Grandfather rented a cottage in the Catskill region of upstate New York. I often visited them for all or major parts of the summer. I spent many hours watching them play pinochle with people vacationing in nearby cottages. They played the card game almost every afternoon and often in the evenings as well. Yet, although they won their share of the hands, win or lose my Grandmother would get into intense and protracted arguments—with her partner (my Grandfather) or with their “opponents” (my aunts or uncles, mostly).

One day, I must have been about 10, I asked my Grandmother why she liked playing pinochle if all it did was aggravate her so much that she constantly got into arguments. “Darling,” she replied, “I don’t care for pinochle at all. It’s the arguments I love.”

No scholar likes having his or her work criticized. However, if we did not enjoy intellectual disagreement or debate we would not “play the game”; we would go into some other line of work.

I believe it is fortunate that scholars enjoy debate. In my view, debate is the cornerstone of scientific progress. Scientific advances, I believe, derive more often from differences among scholars in the interpretation of facts than from the facts themselves.

I am grateful for having had the chance to engage in debate with so distinguished a set of scholars. As I indicated at the beginning of this essay, I feel honored by having been given this opportunity. In addition, I have thoroughly enjoyed it. My hope is that the readers of these essays will find reason to conclude that some progress has been made in the understanding of the issues that the reviewers and I have discussed. If so, then a key goal of *Final Solutions* and, I am certain, of all of the essays in this issue will have been attained.

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KONRAD LORENZ: A PRISONER OF WAR FOR THREE YEARS

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Complete insight into scientific concepts, and especially scientific schools, is impossible without an overview of the history of their origins, without knowledge of the originators of those concepts, and their personal histories and their aspirations. Konrad Lorenz, one of the founders of ethology, had a very complex personality. His contribution to world science is undoubtedly great, but at the same time, his idea that the laws of animal life are directly extrapolative to human society is fallacious.

The fact that Lorenz was a Soviet prisoner of war is not well documented outside of Russia, and in 1989–1990, we searched Soviet archives for information about that period of his life. With the assistance of Soviet archivists, and primarily L. L. Nosyreva, we found the Lorenz file, and unexpectedly, a manuscript of a book by Lorenz.

Lorenz was mobilized into the army on October 10, 1941. He served in a hospital at the rear in Poznan for two years. He was taken prisoner near Vitebsk on June 28, 1944, after having fought for several months in the 2nd Sanitary Company of the 206th Infantry Division. Prisoners were required to fill in questionnaires when captured. He completed two such questionnaires: one on February 14, 1945, in Kirov; the other on February 5, 1947, in a camp in Armenia. The answers to the questions do not seem to have been distorted: the questionnaire in the Kirov camp was completed by the senior sergeant, Toropov, with the assistance of the translator Kocherzhuk; in the Armenian camp, it was completed by the captain of the guard, Karapetyan. It should be noted that he answered these questions many months after he had been taken prisoner, and thus had time to become adapted to the situation and answer them calmly, after careful consideration.

In both questionnaires, Lorenz is given a patronymic in the Russian fashion, i.e., "Adolfovich" after his father's name "Adolf," although Lorenz's middle name was Zakharia (Zachary). He gave his nationality as Austrian and his native language as German. In both questionnaires his

personal signature is clear and begins with "Dr." In the Armenian questionnaire, there is a good photograph of prisoner of war (POW) Lorenz. His answer to the question of "special features" was: height, 183 cm; normal build; light-brown hair; oval face; long nose; grey eyes; in the Kirov questionnaire he describes his eyes as light-blue. He described a scar below his elbow.

He stated that he was born in Vienna in 1903, and gave the village of Altenberg as his family residence. He described his education in the following way: "5 years of public school; 8 years of gymnasium; 5 years of medical school; and 2 years of zoological study." Before he joined the Army, he was a professor of psychophysiology at the Koenigsberg University. He had no military training. When answering the first questionnaire, he said he was a believer (religious); after two years of imprisonment, he answered that he had no denomination. On the first questionnaire he reported that he was a "National Socialist"; on the second, that he was a "candidate of the National Socialist Party." Lorenz never disclaimed his membership in the National Socialist Party, and this modification in his answer is particularly interesting.

He described his social status as an official without property, but in answer to a later question, he indicated that he did own the house in Altenberg. He listed the following as his relatives: 80-year-old father, Adolph Lorenz, a "burgher" by origin and a physician by profession; his brother, Albert, 59 years old, a physician; his wife, Gebhart Margerite Richard, born in 1900; his son, Thomas; and his daughters, Agnes and Dagmar. He lists his son's age as 12 years and the younger daughter as one year old at the time of his mobilization in 1941.

He reported that he visited the following countries before he was imprisoned: America in 1922 for two months on a tour; France, Belgium, Holland, England and Italy: two weeks in each country. In the second questionnaire he adds: Switzerland, Czechoslovakia, Bulgaria, Rumania and Greece.

His position in the army was that of junior doctor; his rank was junior lieutenant. To the question as to whether he surrendered on his own or was taken prisoner, Lorenz answered that he was taken prisoner. He had no military decorations.

The questionnaires give information about his transfer to various camps. Lorenz spent several months at first in two camps near the front; he then went to the Kirov camp, where he worked in a factory (from August, 1944, to November, 1945). Subsequently, he was taken to Armenia, a trip that took half a month. There he worked in three divisions of a labor camp. In 1947, he was transferred to a camp near Moscow, in a town in Krasnogorsk. This was a relatively privileged camp, where the POWs were anti-fascists, or volunteers who fought in the Hungarian or Romanian armies with the Soviet forces. It is likely that Lorenz was sent there on the basis of the good reference given him by the Armenian camp

authority (see below). Lorenz spent relatively little time in Krasnogorsk and in December, 1947, he was repatriated.

Bateson, in his obituary for Lorenz (1989), says that Lorenz survived in the Soviet camps because he ate flies and spiders. We do not know what he experienced in the camps near the front, but we have documents about the life of POWs in the labour camps. Although the life was hard, it was passable for most people. The camps were not "death camps." The authors of the documents, which are marked "top secret," did not believe that the information about those camps would be published. One can visualize the life of Lorenz in those camps: barracks with two to three layers of bunks, 250 to 500 people in each barrack, heated by a stove; the map of the camp shows one lavatory for 10 barracks, "with 20 holes." One can understand the mental and physical hardship of this existence for the "Professor of Psychophysiology." But, living conditions were very difficult for the entire country at that time.

However, Lorenz, being a doctor, was assigned to look after patients in sick quarters, which were probably warmer and cleaner. A considerable portion of the POWs were assigned to "rehabilitation teams." The POWs spent on the average of 26 days in rehabilitation teams, where they were provided with extra food and rest. In the Kirov camp, up to half of the camp population was in such teams for several months running. According to the reports, this large number of prisoners requiring rehabilitation was the result of long periods of encirclement and incessant combat. Normally, only 20% of the prisoners were disabled. The primary cause of disability was the inadequate diet: 2,105 kilocalories per man per day during the worst time of the war. Beginning in 1945, the food quota was increased, and the POW received per day the following: 600 g of brown bread; 90 g of groats; 30 g of fish; 15 g of lard; 15 g of oil; 17 g of sugar; 600 g of potatoes, etc. In the rehabilitation camps, the meat quota was increased to 150 g, sugar to 30 g, and they also received 300 g of milk.

The clothing allowance was also scanty: 2 pairs of undergarments; a field coat; a tunic and wide trousers; boots, shoes or sandals. A mess dish for each 10 persons was issued to soldiers; officers were given a belt, a basin, and a tea kettle for 10 officers.

The file for Lorenz's stay in the Armenian camp contains the following "reference for prisoner of war Lorenz, Konrad Adolf," issued in September, 1947:

Prisoner of war Lorenz is characterized positively. He is disciplined, a conscientious worker, developed politically, actively participating in anti-fascist work, and is trusted and enjoys authority among other POWs. The lectures and reports that he delivered were enjoyed by the POWs.

Prisoner of war Lorenz has visited various states, including . . . He is a broad-minded person in terms of theoretical problems, and is

correctly oriented in politics. Lorenz is a propagandist of the camp division, being involved in mass propaganda work among POWs of the German and Austrian nationalities. He has a command of French and English.

We have no evidence compromising K. A. Lorenz.

In the course of searching the archives, a striking discovery was made: two typed, single-spaced copies of a manuscript in German, one being 211 pages long, and the other, 222. The difference between the two paginations results from the fact that the second copy has a detailed table of contents. The title of the book is *Einführung in die Vergleichende Verhaltensforschung*. There is a quotation from C. O. Whitman at the beginning of the book: "Instincts and organs should be studied from the common viewpoint of their phyletic origin." This book was apparently started as early as the period of his stay in Armenia, and finished and filed in the camp at Krasnogorsk, where Lorenz had the opportunity to type the manuscript. His book, *The Foundations of Ethology*, written in 1981, essentially repeats and enlarges upon the concepts in the manuscript that remained in Russia. Interestingly, his POW experience is present in this later book. When discussing the role of learning, Lorenz cites the example of the ibexes living in quarries in the mountains of Armenia who were not shy of explosions. Apparently, he was remembering his Armenian experience when he worked at the construction site of the Sevan hydroelectric power station.

About fifteen years ago, during a scientific meeting, one of us (V. E. Sokolov) invited Lorenz to visit the USSR. At that time, ethology was actively developing in the USSR, and Lorenz's books were being published. The arrival of Lorenz in the USSR would have undoubtedly aroused great interest among scientists and animal lovers; the latter's interest in animal behaviour essentially sustains the popularity of ethology. Lorenz rejected the invitation. "I have already been in the USSR," he said with a somewhat sad smile.

The fact that he was a member of the Nazi Party, and had been a prisoner in the USSR is well known. This played an important role in the development of ethology, in the USSR; this was used by the opponents of his "non-Pavlovian" approach to the study of animal behaviour. This history also repelled a number of Americans working in the area of comparative psychology. In addition, Lorenz's prewar political sympathies coincided with his speculations about the effects of domestication. Bateson comments on this in the obituary he wrote (1989): "When the Nazis came to power, Lorenz had swum with the tide and in 1940 shockingly wrote an article that dogged him for the rest of his life. He detested the effects of domestication on animal species and he thought (without any evidence) that humans were becoming victims of their own self-domestication. His wish to rid humanity of the impurity matched only

too well the appalling Nazi ideology. . . . After the war, in which Lorenz was to discover with horror the full scale of what the Nazis were really up to, he would have preferred this publication to have been forgotten."

Numerous ethologists regard Lorenz as an opposite to Niko Tinbergen, who participated in the Resistance movement and spent years in a Nazi concentration camp. The two scientists were placed on opposite sides of the barricades by the war and politics, but both of them won the Nobel Prize in 1975 for "investigation into social behaviour of animals." They are both gone now, having died within a short time of each other (Tinbergen on November 21, 1988, and Lorenz on February 27, 1989). The time has come now to objectively analyze their achievements, philosophical and political views, and their life histories.

REFERENCE

- Bateson, P. (1989). Obituary of Konrad Lorenz. *Human Ethology Newsletter*, 5, 3-4.

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